

will continue in spite of all contradiction, and the absolute fact that not a single example is known with certainty to have occurred within the Arctic Circle. However, slight flaws like these do not seriously compromise Capt. Moore, who has certainly succeeded in condensing a greater amount of really valuable information into a small space than any other ornithological writer with whom we are acquainted.

LINKAGES

Linkages. By J. D. C. De Roos. Van Nostrand's Science Series, No. 47. 18mo, 87 pp. (New York, 1879.)

IT is not often that one is able to trace a pedigree with such success as we have been able to achieve in the case of this little book. It appears from the title-page that it is reprinted in its present form from *Van Nostrand's Magazine*, having been translated from a series of articles by M. de Roos in the *Revue Universelle des Mines*. The latter gentleman admits his obligations for the major part of his work to a translation which appeared in the *Revue Scientifique* of November 24, 1874, of the well-known lecture delivered by Prof. Sylvester at the Royal Institution in the same year, which was based on M. Peaucellier's discovery described in the *Nouvelles Annales* of 1873, but contained a large amount of original matter. For the residue, with one exception, to which we shall presently return, M. de Roos appears to be indebted to a paper by M. Liguine, which he mentions without stating where it is to be found, and which, together with a memoir by M. Saint Loup referred to by M. Liguine, is apparently regarded as all that has been written on the subject since the publication of Prof. Sylvester's lecture. M. Liguine's paper is to be found in the *Nouvelles Annales* for 1875; it discusses Prof. Sylvester's lecture, the "contra-parallellogram" of Mr. Hart, the "kite" of Mr. Roberts, and one of Mr. Kempe's earliest linkages. The description of these discoveries of Mr. Hart and Mr. Roberts is stated by M. Liguine to have been obtained from an article by Prof. Sylvester in the *Revue Scientifique* in 1875, but no mention is made of the source from which a knowledge of Mr. Kempe's linkage is derived. There is, however, internal evidence that it is M. Antoine Breguet who published an article in the *Revue Industrielle* early in 1875, which discusses the discoveries of Messrs. Kempe and Hart referred to, and states that the writer's information is derived from their original articles in the *Messenger of Mathematics* of November, 1874.

It is not to be wondered at, after this, that the work before us, though recently published, contains no information of later date than 1874, a time when the theory of linkages was in its infancy. Under such circumstances it would have been more creditable to the editor of "Van Nostrand's Science Series" if, to the statement in his preface to M. de Roos's book that "the subject has not even yet received the attention which is fully its due," he had added the qualifying words, "though very much more has been done than is contained in M. de Roos's work, which is at the present time, from the rapid advance which has been made during the past five years, somewhat antiquated." As a matter of fact, not the slightest hint is conveyed, from the beginning of the work to the

end, that it does other than represent the present state of the science of linkages.

The book bears no signs, as far as editor and translator are concerned, of being only vol. i., but M. de Roos does conclude with a promise in a "future note" to discuss a "new element," of which a diagram is given, briefly noticed (not described) in a paragraph containing a misleading misprint of O A . A B for O A . O B, which in the absence of the "future note" may make it difficult for the reader to understand what the new element is. The translator proposes to name it the "Element of De Roos," "in honour of its inventor;" an examination in the light of the correction we have indicated will, however, show that whatever claim to novelty might have been advanced five years ago (though we feel somewhat doubtful whether even then the "discovery" of a combination of half a "Peaucellier" and half a "Hart" would have entitled the discoverer to have his name affixed to it), at the present time when more general linkages have been discovered of which it is only a particular case, none such could be sustained.

The bulk of the volume consists of applications of the Peaucellier inverter and Prof. Sylvester's modifications to the description of curves, the extraction of roots, &c. These, though decidedly interesting, would in many cases be superseded at the present time by less cumbersome methods. The pages are plentifully supplied with diagrams, which are, however, occasionally marred by the omission of links. This is particularly to be regretted in the case of Fig. 48, which exhibits one mode of practically applying Peaucellier's parallel-motion to a beam-engine. It may not be uninteresting to note that this method is the same as that employed by M. Peaucellier in a model furnished by him to the Conservatoire des Arts et Métiers in Paris.

We cannot but regret that what appears to be a useful science series should be marred by the introduction of a work which, possibly through no fault of the author, must by its antiquity mislead its readers as to what has been and remains to be done on the interesting subject of which it treats.

OUR BOOK SHELF

The American Entomologist. New Series, No. 1. January, 1880. C. V. Riley, Editor; A. S. Fuller, Assistant Editor. (New York: Max Jaegerhuber.)

We are glad to welcome an old friend in an old face, after nine years' absence. The idea of this journal originated from that lamented entomological genius the late B. D. Walsh, in the form of the *Practical Entomologist*. This developed into the *American Entomologist*, and to this was subsequently added, as part of the title, and *Botanist*. The *American Entomologist* is now resuscitated, under its former editor, the energetic Prof. C. V. Riley, and bids fair to be a success. Purely descriptive entomology evidently finds little favour in the eyes of the editors, "descriptive" papers being limited to one page in each number, or if more extended, the cost is to be paid by the author, and the space so occupied is to be supplementary. Thus, the aims of the journal are almost exclusively biological and economic. It is just possible this idea may, at some future time, be slightly modified. The editors crave that indulgence usually accorded to first appearances, but, having no doubt fully in mind the fact that one of them is an old stager, they have produced a "first" number of a most varied and useful

nature, full of information on the habits of a multitude of North American insects, good, bad, and indifferent, as to the characters borne by them. There are also several excellent woodcuts; yet we fancy some of these are old friends. In future numbers we hope to see more originality in this respect, because the constant reproduction of the same illustrations in different works, engenders a suspicion, with those uncharitably inclined, that the text may be sometimes written up to the illustrations, and the latter not made subservient to the former, as ought to be the case. We shall watch the progress of this journal with appreciative interest. The list of names of those who have promised occasional contributions includes most of the leading American entomologists.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

A Museum Conference

You did me the honour, about two years ago, of inserting an unsigned communication pointing out the extreme desirability of a conference of officials connected with museums and galleries of art throughout the country. At the time the subject received a good deal of attention from various quarters, and the numerous advantages which might be derived from such a meeting commended the suggestion to all who wrote on the subject. No one, however, ventured to make a practical move in the matter at the time, and the subject consequently dropped.

Further consideration and growing experience have deepened my conviction of the utility of the conference scheme; and as I have reason to believe I am not singular in that experience, I desire now to see some effort made to bring the question to a practical issue. With this view I shall be glad to co-operate with other museum officials who feel inclined to take part in the preliminary work of organising a conference of those interested in museums and art galleries. As to where, when, and how the conference should be held, I do not wish to offer a single suggestion which might anticipate future consideration. Neither do I consider it necessary to occupy your space with any statement as to the great and manifold advantages which ought to accrue to our scattered exhibitional institutions by a union such as might be formed. These are surely too manifest to every individual who has to do with any museum, especially in the provinces.

I hope this question will now be taken up heartily and energetically by all interested; and while I would beg that you may give space for the suggestions which others may wish to make through the medium of NATURE, I shall be glad to enter into correspondence with those who may address me privately.

Kelvingrove Museum, Glasgow

JAS. PATON

The Himalayan Ranges

I HAD not intended to notice Mr. Trelawney Saunders's remarks on Mr. Medlicott and myself as the authors of the "Indian Geological Manual" (NATURE, vol. xxi. p. 96). As, however, Mr. Medlicott's reply (*ante*, p. 301) has been misinterpreted by Mr. Saunders, and as the latter has, in his rejoinder (*ante*, p. 347), brought a specific charge of omission which can, I think, be shown to be unfounded, against a portion of the "Manual" written by myself, I am obliged to take part in the discussion.

In Mr. T. Saunders's original paper (*l.c.*, p. 96) read before the British Association, two objections were raised to the views on physical geography adopted by the authors of the "Manual." The second of these objections referred to the limits of the Himalayan range, which we did not represent as extending west of the Indus. Mr. Saunders must have read very little of the "Manual," or he would have seen that this limit was not absolutely defined; on the contrary, at p. 518, it is expressly termed provisional. As Mr. Medlicott has shown, there is a good geological reason for the limit adopted; but another cause, of perhaps even more importance, is that this limit coincides

with the boundaries of the area described in the work. I cannot enter into the question here, but the fact is, there are just as good reasons for making the Himalayan range terminate at the Jhelum, if not even farther east, as for prolonging it beyond the Indus.

The first objection was couched in much stronger language. Mr. Trelawney Saunders had represented the Himalayas as consisting of two chains; we were accused of having adopted an "antiquated theory." No reference was given, but from the context it was evident that this "antiquated theory" consisted in representing the range on a skeleton map by a single line along the water-shed or water-parting (I will employ the latter term to prevent any risk of misconception). Mr. T. Saunders says (*l.c.*, p. 96) that they (*i.e.*, Mr. Medlicott and myself) "do not condescend to any reason for this conclusion." This is not quite correct. If Mr. Saunders had "condescended" to read the two and a half pages in the introduction of the "Manual" relating to the physical geography of the Himalayas, he would have found a reason on p. x.

Mr. Medlicott very justly pointed out that the reason for omitting the representation of a second chain was due to the irrelevancy of the question whether there are one or two chains to the matter in hand, that is, to the physical geography of India as related to the geology. Mr. Saunders has quite misinterpreted Mr. Medlicott's meaning when he says (p. 348): "Mr. Medlicott contends that the omission was due to the irrelevancy of the great range to the matter in hand." Of course Mr. Medlicott means nothing of the kind.

In his letter just referred to, Mr. Saunders writes thus:—

"But my complaint was based, not on my delineation, but on a trigonometrical survey, and it was caused by a *description*, not of the geology, but of the physical geography of India, in connection with a map of its hill-ranges, that has nothing geological about it. It is in this expressly geographical part of the 'Manual' that I find the greatest range of snowy peaks in the world omitted from a *geographical notice* and delineation of the Himalaya."

The italics are mine. Again no reference is given, but the remarks quoted can only apply to the description of the physical geography, accompanied by a skeleton map, in the Introduction to the "Manual." In this description the "geographical notice" of the Himalayas occupies barely two and a half pages. One would have thought that before writing the sentence I have quoted the writer would at least have read this small amount of letterpress. Yet I scarcely think Mr. Trelawney Saunders can have done so, or he could scarcely have overlooked the following passage at the bottom of p. ix. and upper part of p. x.

"Many geographers distinguish two parallel ranges from the neighbourhood of Simla to the eastward: the snowy range proper, formed of the highest peaks; and a more northern ridge, forming the water-shed between the Tibetan plain and the rivers running to the plains of India."

To save space I quote no more, but I am convinced that any one who will refer to the two and a half pages headed "Himalaya," in the Introduction to the "Manual," will see that Mr. Saunders is quite in error in saying that the main range is ignored.

As Mr. Trelawney Saunders has not understood Mr. Medlicott, I can only hope that the following explanation may be clearer:—

In his original paper and in that in the *Geographical Magazine* for 1877, pp. 175, 176, Mr. Saunders contends that the Himalaya south of the Sanpu and upper Indus consists of two "chains" (these are alternately called chains and ranges). The southern chain is formed by the line of great peaks, the northern by the water-parting between the drainage areas of the Upper Indus, Upper Sutlej, and Sanpu on the northern side, and various rivers running to the plains of India on the southern.

Now it is manifest that this division of the Himalayas into two chains is due to the fact that two different, and to some extent irreconcilable, definitions are adopted for the term "chain" in the two instances. Mr. Saunders's southern chain is a line of great peaks, but is not a continuous water-parting; his northern chain is a continuous, or almost continuous, water-parting, but is not a line of great peaks. It has never been shown that the two are distinct axes or lines of elevation; on the contrary, all the evidence we possess tends to show that both are due to one great fold of the earth's surface, and until these northern and southern chains are shown to be of diverse origin, it is perfectly reasonable to decline to accept the two distinct acceptations of the term "chain," and it is consequently perfectly correct